The plates illustrate most fully the points made out, in many cases a longitudinal section of the skull of the animal at birth being printed in red over a drawing of one of adult age, both drawings having been reduced to scales which render comparisons of form possible.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Suggested Nomenclature of Radiant Energy.

HAVING recently had occasion to develop the first principles of the theory of inter-stellar radiation, I soon felt the want of some short and convenient word to express that form of ethereal wave-effect known as "radiant energy," "radiant heat," "light," "rays of the spectrum," &c. Radiant energy is doubtless the most accurate of these expressions, but it is subject to the objection of being a description rather than a name. The nomenclature of the subject has come down from a time when it was supposed that there were three distinct kinds of rays in the spectrum, severally known as light, heat, and actinic rays. It is, I believe, not much more than half a century since several eminent physicists and teachers supposed that the heat rays of the spectrum could be separated from the light rays having equal refrangibility by the absorption of a transparent medium; and that even the light rays of different colours might be separated in the same way. I cannot but think that the general understanding and application of the now received theory of the subject, which recognises in this form of energy no differences of kind except wave-length, has been materially retarded by the want of a corresponding nomenclature.

The use of the word "light" for ethereal waves having a length between certain definite limits, while there is no corresponding word for other waves, is evidently unscientific. Notwithstanding the great practical usefulness of light, its distinctive property of affecting the optic nerve in a certain way can claim only a secondary place in physics. Indeed, it has long seemed to me that the banishment of the word "light" from physics was a desideratum.

After various attempts I hit upon the very simple term *radiance*, as one which seemed well-fitted to supply the want in question. The vague and poetic idea hitherto associated with it is an advantage, because it enables us to adapt it to the case in hand with greater readiness than we could adapt a word which already had some well-defined meaning. Shakespeare speaks of the "sacred radiance of the sun"; while Milton describes the Deity as "Girt with omnipotence, with radiance crowned." We can thus adopt the word to express scientifically what we now consider to be electro-magnetic waves, or ethereal waves, without that clashing of ideas which might arise from making a new application of an old word, and without the awkwardness of coining a new one.

The necessary derivatives and compounds of the word can be formed with as much ease as we should expect in the case. The verb "radiate" will mean to emit radiance. I do not think any confusion will arise if we use the word "illuminate" to signify the throwing of radiance upon a material body, although in ordinary language it implies light. Possibly the extent to which it is used in a tropical sense may facilitate the widening of its literal meaning. *Radiometry* would mean the measure of radiance, and an instrument for effecting such a measure would naturally be called a radiometer. It is perhaps unfortunate that the instrument in question should then assume the name of Crookes' beautiful little instrument, but an apology may be found in the fact that the latter has not been used for the purpose of exact measurement. The use of the word "radiometry" offers no such difficulty.

I am still a little perplexed for a word which shall express the quality hitherto called transparency, diathermancy, &c. Apparently we have no alternative but to continue the use of one of these objectionable words, or invent some such new word as transradiant, or transradious.

The proper measure of radiance, and the only measure which can be regarded as of real importance in physics, should be the amount of energy radiated in unit time. This measure is equivalent to that of heat generated in unit time in the absorption of radiance by a perfectly black body. If we reflect that this, and this alone, measures the actual loss of internal energy by a radiating body of any kind, whether ball of iron in a laboratory, planet, star, or nebula, the importance of some simple nomenclature of measurement will be evident. I should be much pleased if physicists would find by actual trial whether the use of the proposed words comes as natural to them as it has to me.

SIMON NEWCOMB.

The Postal Transmission of Natural History Specimens.

IT has always been recognised that scientific research is greatly furthered by the exchange of the various objects with which that research is concerned. For the transmission of objects of Natural History from one country to another, the mails have offered a cheap, speedy, and trustworthy means. Heretofore, through the laxity with which the regulations on the subject have been enforced, it has been possible to enter such objects in the mails of the Universal Postal Union as samples of merchandise and under the rates of postage therefor. From official information lately received from the Post Office Department of the United States it appears that such a rating is entirely unauthorised by existing provisions, and that objects of Natural History may be mailed to countries of the Union only, at the rates required for letters. The United States Post Office Department also stated that it had recently submitted a proposition to the countries composing the Postal Union to modify the regulations so that such specimens might be received into the mails at the same rates as samples of merchandise, but that a sufficient number of those countries had voted against the proposition to defeat it.

This Academy has therefore resolved to address the various scientific bodies, with which it is in communication, in those countries whose Governments have voted against the proposition, and to request those scientific bodies to memorialise their respective Governments in favour of the same.

The Governments of Austria, Bolivia, British India, Canada, Germany, Great Britain, Guatemala, Hungary, Japan, Norway, Portugal, Russia, Spain, Sweden, Tunis, Uruguay, and Venezuela having voted in the negative, this Academy respectfully requests the favourable consideration of this question by scientific societies, and begs that they take such steps as they deem advisable to inform the Postal authorities of their respective Governments of the manifest advantages to scientific research which would result from the adoption of the proposed modification, and to request those authorities to take such steps as may result in the adoption of the same.

The letter rate for postage (Universal Postal Union) is ten times that required for samples of merchandise; such a rate for specimens of Natural History is virtually prohibitive.

This Academy would respectfully urge upon scientific societies prompt action in this matter, if it meets with that approval which we so strongly desire.

ISAAC J. WISTAR, *President.*
EDW. J. NOLAN, *Recording Secretary.*

Philadelphia, November 14.

Flame.

HOWEVER thoroughly a B.A. audience may have allowed Prof. Smithells, by means of his beautiful experimental demonstrations, to hypnotise them into unquestioning belief in his conclusions, those who read the account of his lecture in the pages of NATURE will not all be equally disinclined to question the validity of some of his arguments.

To tell us that Dalton, as a matter of fact, long ago *settled* the question as to which has the preference—the carbon or the hydrogen—when a hydrocarbon is burnt with insufficient oxygen, is, after all, but to appeal to the gallery; and this and other conclusions arrived at by Prof. Smithells appear to me to involve the use of that process of circular reasoning which consists in taking for granted that which is to be proved—a method which at the present day finds such favour in certain quarters.

As I discussed this matter somewhat in detail in a correspondence with Sir G. G. Stokes last year (Chem. Soc. Proceedings, 1892, No. 106, p. 22), it is unnecessary to go fully into it now. Any number of analyses showing the presence of hydrogen in the products of combustion may be quoted without materially ad-

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vancing the settlement of the question. In my opinion, there is no improbability inherent in the assumption that hydrogen is but a secondary product, resulting from the interaction of the primary products—water and either carbon or carbon monoxide. The rate at which the interactions take place in flames are such, and the conditions are such, that the products *collected* are probably far from being the *products of the initial interchanges*, as indeed Prof. Smithells himself admits to be the case. It is scarcely likely that the settlement of such a question can ever be achieved by direct observation. Our ultimate views on the nature of the changes occurring in flames must depend on the gradual growth of a true understanding of the nature of chemical interchanges in general, and especially in gases.

I am inclined to take the same view with reference to Davy's explanation of the luminosity of flame. If eventually, as is not improbable, we come to regard the expressions *chemical interchange* and *electrolysis* as interchangeable equivalent terms, much more will have to be said on behalf of Frankland's hypothesis. I had the good fortune to attend the philosophic lectures at the Royal Institution in which Frankland, in 1868, first fully stated his views on this subject, illustrating his arguments by a series of most striking experiments. No course of lectures ever impressed me more, and to the present day I have the most vivid recollection of all that passed. An account af the lectures was published in the *Journal of Gas Lighting* at the time of their delivery. It has always appeared to me that Frankland's arguments are of a most weighty character, and that owing to their appearance in an obscure publication they have never yet been sufficiently widely considered.

The study of flame affords problems of the highest interest and importance, but of proportionate complexity and difficulty. There is little doubt, however, that we are inclined to take too narrow a view of this as of many other inquiries—that we have an unreasoning belief in what we are pleased to call facts, forgetting that these same "facts" are but phenomena interpreted by our own limited intelligence. On studying the views that have been taken at various times, it is only too obvious that fashion is not confined only to garments, nor is dogma the exclusive privilege of theologians; and it is time that we realised that very many of our conclusions regarding chemical interchanges are but the crudest dogmas, based on a thoroughly superficial consideration of the phenomena. If we are to deserve the title of scientific workers—workers exact in deed, thought, and word—we must be far more careful in the choice of our language, and guarded in our conclusions.

HENRY E. ARMSTRONG.

"Geology in Nubibus."

SIR HENRY HOWORTH wishes to continue the discussion of glaciation in the pages of NATURE, but I find in his last letter very good reason why this cannot be done. No discussion can lead to definite results unless the parties to it accept as data what they themselves have recently and deliberately admitted. But when I stated that the Rhone glacier *did* reach the Jura, and deposit on it erratic blocks between Geneva and Soleure, I did so because it was one of the data already admitted by Sir H. Howorth. In his "Glacial Nightmare," pp. 169-173, he gives a full summary of Charpentier's first memoir on the erratic blocks of Switzerland, describing the glacial phenomena exhibited along the whole course of the old glaciers from the Alps to the Jura, and showing that they "even climbed that range and went over to the other side of it." Sir H. Howorth then says: "I have quoted at considerable length from this excellent memoir, because I look upon it as having definitely applied *inductive methods to this question* with results which are for the most part *sound and unanswerable*." (Italics mine.) In the same chapter (pp. 195-202) Charpentier's second memoir is summarised still more fully, and his general conclusion is thus quoted: "It goes without saying that not only all the valleys of the Valais were filled with ice up to a certain height, but that all lower Switzerland, in which we find the erratic débris of the Rhone valley, must have been covered by the same glacier. Consequently all the country between the Alps and the Jura, and between the environs of Geneva and those of Soleure has been the bed of a glacier." Agassiz and other writers are quoted as giving further evidence of the same kind. Nowhere in the whole of this chapter can I find a single objection to the conclusions of the chief writers quoted, and the concluding paragraph, at p. 208, frankly accepts them. It declares that they are supported by "every form of converging evidence," and that—"So far there is no question at issue." Yet, when I take these same conclusions of Charpentier as admitted data, Sir H Howorth says: "This form of dogmatic argument is assuredly incomprehensible!" Charpentier's proof that the Rhone glacier reached Soleure, was, a year ago, "sound and unanswerable," and was an example of "definitely applied inductive methods"; but when I accept these same results as something to reason upon, I am told that I am making use of "hypotheses outside the laws of nature." I have now justified my opening statement that a discussion carried on in this manner can serve no useful purpose.

ALFRED R. WALLACE.

Correlation of Magnetic and Solar Phenomena.

IN Mr. Ellis' letter on this subject (NATURE vol. xlix. pp. 30), he says:—

"To sum up, the points of the matter may be thus stated:—(1) The solar outburst in 1859 was seen independently by two observers: the fact of its occurrence seems therefore undoubted. (2) The corresponding magnetic movement was small. (3) Many greater magnetic movements have since occurred. (4) No corresponding solar manifestation has been again seen, although the sun has since been so closely watched."

Now, in the year 1882, I was acting as assistant to the Solar Physics Committee, and on November 17 there was a dense fog, so that it was not possible to take the usual solar observations. Mr. Lockyer was present in the morning, and then left for some reason; after he had gone, a telegram came for him; he returned late in the afternoon, and sent for me, told me the telegram was from Mr. Preece, of the Post Office, asking him whether there was a solar disturbance, as there was such a violent electrical storm raging, that communication had been cut off from the continent, and that it was difficult to maintain communication in England. I at once went to the instruments, and as the fog cleared just before sundown, was able to ascertain that there was a large group of spots near the sun's meridian, attended with most violent uprushes of luminous matter; indeed, if my memory serves me aright, it was the most violent disturbance I saw during the whole of my observations, extending from 1879 to 1886. On reporting to Mr. Lockyer, he said we should probably see an aurora in the evening; and as soon as it was dark, there was a most brilliant auroral display that exhibited some quite new features (NATURE, vol. xxvii. pp. 82 *et seq.*) Doubtless, had this spot been kept under observation, luminous outbursts similar to those observed by Carrington and Hodgson would have been seen; indeed, Mr. Whipple's letter (*loc. cit.* p. 83) seems to contain such an observation.

I believe, but am not quite sure, as the records of the observations are in Mr. Lockyer's possession, that it was in this spot that he and I first noticed that some of the so-called iron lines in the spot spectrum were in motion, while others were not.

H. A. LAWRANCE.

Gunnersbury, November 19.

New Variable Star in Andromeda.

A STAR that should be added to the list of variables is + 26°43, of the Bonn *Durchmusterung*, in which work its magnitude is given as 8·7. In reply to a letter of mine, in which I expressed a doubt as to this star's existence, Dr. Küstner, of Bonn, informed me that although he had on the 7th of this month looked in vain for the star with the 6-inch refractor of Bonn Observatory, yet it seemed pretty certain that a star had twice been observed in the specified place in September, 1855. I have subsequently been informed by Sir Robert Ball, that the star was twice observed at Cambridge (England) in 1878. The dates and places of the various observations, as well as the estimated magnitudes, are:—

Sept. 7, 1855, Bonn, 9·0 (but perhaps 9·2).
Sept. 10, 1855, Bonn, 8·3.
Nov. 29, 1878, Cambridge, 8·7.
Dec. 11, 1878, Cambridge, 8·7.

The star's mean place for 1894·0 is

R.A. 0h. 16m. 51·3s.
Decl. +26° 24′ 27″

THOMAS D. ANDERSON.

21 East Claremont Street, Edinburgh, November 22.

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